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TRENDS OF MODERN BIOLOGY[1]

I

AN occasion such as this is thought-provoking. Why should anybody endow a chair of biology? When I began the study of the subject a little more than a quarter of a century ago such things were not done. In most of our large universities biology had a fairly secure position, but in all but a very few of the small colleges, at one of which I am proud to say I had the privilege to study, if present at all it was so distinctly only on sufferance. Much doubt existed and was often expressed as to whether this novel subject had any disciplinary value in the training of the youthful mind, or had any particular cultural worth in the producing of better citizens. Those of us who were irresistibly lured, by the fascination of the wonderful field opened to our vision, to spend most of our time in the biological laboratory, were looked upon by our fellow collegians as queer freaks of nature, and would certainly have been called Bolsheviks had that overworked appellation been current verbal coin in those days. For the subject distinctly lacked respectability. It was thought by those who pursued the classics or other orthodox lines of educational conduct to be a messy business, was known to be smelly, and was generally held to be low. This attitude inevitably called forth a defense reaction on the part of its callow devotees, which resulted in distinctly worse messes and smells than were really requisite for the successful pursuit of knowledge in the field.

Now all this has changed. Biology has come

[1] Papers from the Department of Biometry and Vital Statistics, School of Hygiene and Public Health, Johns Hopkins University. No. 80.

An address delivered at Mount Union College, Alliance, Ohio, October 20, 1922, on the occasion of the dedication of the Milton J. Lichty Chair of Biology in that college.

into its own, and the security of its position in the educational world can not be shaken even by so doughty a champion of the powers of intellectual darkness as Mr. Bryan. What has happened in these twenty-five years in biology? And what of the present and of the future? Can we find in the efforts and achievements in this field due warrant for that intellectual respectability that biology has now gained, and for that clear faith in the future which is implied in Dr. John A. Lichty's splendid endowment which we are here gathered to dedicate?

Perhaps as good a method as any of getting light on this matter will be to attempt a review of the major trends of biology in the past and the present. In doing this we shall find that in every case these trends of thought and research have been responses to some quite naïve and simple bit of intellectual curiosity, of the sort likely to arise in a child's mind, if he turned his thought at all to living nature about him. It may fairly be said that up to the time of Darwin and Wallace and the "Origin of the Species," all biology busied itself with the answering of one phase or another of the following two naïve questions:

First, how many and what different kinds of animals and plants exist, or have existed, on the face of the earth.

Second, regarding living animals and plants as ingenious and complex contrivances, but after all not fundamentally unlike other contrivances, how are they put together and how do they work?

Every boy and girl who collects butterflies, or who pulls a wasp to pieces in order to locate and with safety observe the behavior of its "stinger," is in a rough and ready way repeating in his own development the history of the growth of our present knowledge of biology. He is trying on the one hand to get together a collection of the different kinds of living things about him, and on the other hand to inform himself as to their structures and functions.

Since the publication of the "Origin of Species" a third question, essentially just as naïve, but less easy to deal with objectively and practically, has occupied a great part of the attention and effort of biologists. But that it indicates a sort of intellectual curiosity not essentially one bit more sophisticated than the other two, is plain enough if we remember that all peoples to the remotest historical time, and including even savages, have not only thought about it, but also have had theories about it. This question we may put in this way:

Third, whence, why, and how came the animals and plants which inhabit the earth to be here at all?

It is, as I have said, in an attempt to answer these three questions, in some one or other of their aspects, that all we know to-day about biology has developed and grown. It is an impressive fact, recently discussed with great brilliancy by James Harvey Robinson[2] that always in science, biology no less than all the rest, the motivating problems which have led to the advancement of knowledge have been simple naïve questions about quite commonplace things. He says:

> Those to whom a commonplace appears to be most extraordinary are very rare, but they are very precious, since they and they alone have made our minds. It is they who have through hundreds of thousands of years gradually enriched human thought and widened the gap that separates man from his animal congeners. Without them the mind as we know it would never have come into existence. They are the creators of human intelligence. The mass of mankind must perforce wait for some specially wide-eyed individual to point out to them what they have hitherto accepted as a matter of routine or failed altogether to notice. These mind-makers are the questioners and seers. We classify them roughly as poets, religious leaders, moralists, story-tellers, philosophers, theologians, artists, scientists, inventors. They all are discoverers and pointers-out. What eludes the attention of others catches theirs. They form the noble band of wonderers. Commonly unnoticed things excite a strange and compelling curiosity in them, and each new question sets them on a new quest. They see where others are blind, they hear where others are deaf. They point out profundities, complexities, involutions, analogies, differences and dependencies where everything had seemed as plain as a pike staff.

Robinson, in what I have quoted, lays em-

[2] Robinson, J. H.: "The Humanizing of Knowledge," Science, N. S., Vol. 56, pp. 89-100, 1922.

phasis on the kind of man who sees the problem. Perhaps it may help by ever so little in the production of such men in this laboratory which we are starting on an enlarged career of usefulness to-day, to emphasize the importance for success in biology of being simple-minded.

II

Our first question about the different kinds of living things which people this earth led to the important branch of biology which is called taxonomy or classification. This was for a long time the dominant trend of the subject. The first step toward a proper knowledge of the phenomenal world is obviously to get the phenomena classified in an orderly scheme. In biology this takes the practical form of getting different kinds of plants and animals described, named and classified. Linnæus was able to classify all the plants and animals known up to 1735. Nowadays no one person would think of attempting so colossal a task, and if he did would fail by virtue of the inadequacy of the human life span. Instead we find the worker in the branch of biology to-day devoting his life to one, or at most a few, groups of animals.

From its once dominant position taxonomy has apparently fallen to-day, one must reluctantly confess, into rather lower repute in the mind of the general biological public. Neither our professors nor our students of biology appear, with a few brilliant exceptions, to be interested in it. One forms the impression that perhaps four fifths of the Ph.D.'s turned out in zoology at the present time not only never have, but probably never will, for themselves, identify an animal strange to them, and as for deciding whether the unknown creature has been previously described, or placing it in proper taxonomic relation to its nearest relatives, such a problem would be as far beyond their powers as it is beyond their desires. By a curious paradox many modern biologists take precisely that attitude towards and about the living world around them in the practical conduct of their every day working life, which they would logically be expected to take if it were their deepest conviction that each living thing were the product of an act of special creation—God-given and therefore not to be worried about—and that such a process as evolution had never occurred.

Yet it is beyond question that if a young man embarking on a biological career has a desire to make an enduring contribution to knowledge, of permanent value, and incapable of being upset by any future developments of the subject, his best chance of doing this laudable thing is by becoming a careful, accurate taxonomist. If he describes accurately, carefully and completely a hitherto undescribed species of animal or plant, in such a way that any one who reads carefully the description can recognize and identify the thing described, he has chiseled for himself an indelible record in the history of man's intellectual progress.

Some there are who will argue that while what has just been said may be true, the niche in the tablets of history carved in this way is too slight to be of any significance, that, in short, systematic or taxonomic work has only a small and unimportant intellectual content, as compared with other sorts of biological study. Such a view of the case seems to me to be singularly lacking in vision. It means that the commonplace elements in taxonomic work have been allowed to overwhelm in their view its broad and deep significance. The labors of the taxonomists have alone given us such picture as we have of the inter-relationships, unity in diversity, and diversity in unity, of animate nature as a whole. It is the systematist who has furnished the bricks with which the whole structure of biological knowledge has been reared. Without his labors the fact of organic evolution could scarcely have been perceived, and it is he who to-day really sets the basic problems for the geneticist and the student of experimental evolution. His facts are the raw material from which the laws of organic evolution, in the sense that we speak of physical laws, must be worked out. An example of what is apparently a real law of organic evolution, deduced directly from the simplest taxonomic statistics, is found in the fact that the sizes of genera of plants and animals, as measured by the number of species each contains, are not distributed in frequency accord-

ing to the normal curve of error, as most chance determined phenomena are, but instead obey with extraordinary exactness, as has been shown by Willis and Yule,[3] the rule that the logarithms of the *frequency* of genera plotted to the logarithms of the *size* of the same genera (*i. e.,* the number of species in each), give a straight line.

It is with much satisfaction that we find the leading exponent of the reigning mode in present-day biology, Bateson,[4] saying of taxonomy:

> I had expected that genetics would provide at once common ground for the systematist and the laboratory worker. This hope has been disappointed. Each still keeps apart. Systematic literature grows precisely as if the genetical discoveries had never been made and the geneticists more and more withdraw each into his special "claim"—a most lamentable result. Both are to blame. If we can not persuade the systematists to come to us, at least we can go to them. They too have built up a vast edifice of knowledge which they are willing to share with us, and which we greatly need. They too have never lost that longing for the truth about evolution which to men of my date is the salt of biology, and the impulse which made us biologists. It is from them that the raw materials for our researches are to be drawn, which alone can give catholicity and breadth to our studies. We and the systematists have to devise a common language.
>
> The separation between the laboratory men and the systematists already imperils the work. I might almost say the sanity, of both. The systematists will feel the ground fall from beneath their feet, when they learn and realize what genetics has accomplished, and we close students of specially chosen examples may find our eyes dazzled and blinded when we look up from our work-tables to contemplate the brilliant vision of the natural world in its boundless complexity.

It seems probable that we shall before long witness a return to a saner attitude than has prevailed in the last quarter of a century in regard to systematic zoology and botany; and in the training of our students, by not beginning specialization too soon and too violently, give them a more adequate conception than they now get of the orderliness and the diversity which together characterize animate nature as a whole.

III

The dominant mode in biology in my student days was morphology. I was nurtured on the somewhat arid problems of vertebrate cephalogenesis and the components of the cranial nerves. Probably few students in these days are excited by such problems. A vague awareness that there are such things as cranial nerves no doubt suffices and everyone is just as happy. The whole subject of pure morphology, as it was cultivated twenty-five years ago, seems singularly sterile now. It was a highly developed discipline, with a set of rules as rigid, and also be it said about as soul-stirring, as those of the Greek grammar. In its fine spun theories about homology, metamerism and the like, biology got off on a wrong track, which, as is now practically universally admitted, had only a blind ending.

But this does not mean, as those of the younger generation are apt rashly to conclude, that the old morphology was of no value. Intrinsically it was of great value. Few things will transcend in importance in the study of biology, the finding out of all that can be learned about the way in which living machines are put together. As long as this purely descriptive purpose was the primary and essential object of morphological study, all was well. The business only began to go bankrupt when it took on an essentially metaphysical purpose, and a logically bad, not to say hopeless one, at that. For what the pure morphologists of the eighties and early nineties were trying to do was to infer from purely static phenomena (the intimate structure of the body) the dynamic relations in a course of events (organic evolution). Such a task would have been perceived to be hopeless long before it was, except for the seductive lure of certain rules by which the game was played, which rules (such as ontogenetic recapitulation

3 Willis, J. C., and Yule, G. U.: "Some Statistics of Evolution and Geographical Distribution in Plants and Animals, and Their Significance," *Nature,* February 9, 1922, pp. 177-179.

4 Bateson, W.: "Evolutionary Faith and Modern Doubts," SCIENCE, N. S., Vol. 55, pp. 55-61, 1922.

of phylogeny, certain aspects of homology, etc.) were mistakenly supposed to be natural laws, whereas in point of fact, at the best they were only imperfect expressions of certain inherent necessities of the philosophic principle of organization, and at the worst just plain buncombe.

It is unfortunate that in the reaction against this sort of thing which has occurred in the last quarter-century the pendulum has swung so far as to deprive the present day student of biology of a good deal of the exact rigid morphological training that he got in earlier days. There never has been any better training for hand and eye and mind than that which went with the getting of an adequate understanding of the comparative anatomy of the vertebrates, no matter what field of biology the student subsequently entered upon as a specialty. So generally inadequate is the training in this field, now, I am told, that several of our best medical schools have found it necessary to devote a not inconsiderable part of the time allotted to anatomy in the medical curriculum, to the study of vertebrate comparative anatomy, because it is essential to the right understanding of human anatomy, and the students do not have it when they come, although they have the bachelor's degree and have been required to take biology.

We have seen, in the brief sketch which has so far been given of the course of biological events, that two trends of thought and research that were formerly of major importance have on the whole fallen somewhat into a state of desuetude. It will pay us to inquire a little more carefully into the reasons for this change of interest and esteem, because otherwise we are apt to reach the erroneous conclusion that taxonomy and morphology were never of any real importance or significance in the development of human knowledge, and that our forefathers only deluded themselves in thinking that they were. The fundamental reason for the decline in the cultivation of these two disciplines has already been touched upon. It is found in the fact that taxonomy and morphology, as originally practised in their pristine purity, dealt solely with static aspects of vital phenomena. Now the only thing of really compelling interest and significance about life is its dynamic character. Organisms live and do things. It is only this which makes them more interesting than bricks or paving stones. But by a curious quirk of the evolution of intellectual matters, the only group of people, before the publication of the "Origin of Species," who, as a group if they perceived this somewhat obvious fact, did anything about it, were the physiologists.

The historical development of physiology was bound up with and a part of that of medicine, rather than what we now call general biology. The first systematic treatise professedly dealing with physiology as an integral part of general biology was Claude Bernard's "Physiologie générale" and appeared only in 1872. The significance of this is that, in the main, and with only a few notable exceptions, those who prior to that time had been interested in physiology had been almost wholly concerned with workings of the mechanisms solely of the human body, and even in this somewhat narrow field, the significance of the findings for the science and art of medicine held the foremost place in esteem. All this has, of course, changed with the considerable development during the last quarter of a century, of general physiology under the leadership of such men as Loeb in this country, Bayliss in England, and Verworn in Germany.

But at its best physiology concerns itself chiefly with only certain of the *internal* dynamic phenomena of living things, and this is only a small part of the sum total of the activities which constitute life. That all biology should primarily be concerned with dynamic matters was first brought powerfully to the attention of thinking men by Darwin. The significance of Charles Darwin's work upon the intellectual development of his and subsequent times has been variously described and estimated. If we go down to real fundamentals it seems to me that we must conclude that one of the most important elements, at least, lies in the making it so plain as never again to be misunderstood, that the essential problems of biology are questions of dynamic relationships and not of static phenomena.

The immediate effect of Darwin's work, at

least so far as zoology was concerned, was a curious one. It led to an enormous development of research in what is perhaps the most essentially static branch of biology, namely, pure morphology. The process of reasoning was something like this. Since evolution leaves a record of its progress in the structures of animals, by studying these structures intensively it ought to be possible to reconstruct not only the course, but even also the method, of evolution. Von Baer's so-called law, to the effect that ontogeny repeats phylogeny, was held to be the key that would unlock all the secret places of organic evolution, and the biological world went more or less mad over embryology.

But as has already been pointed out, this line of attack proved to be sterile, so far as the problem of evolution is concerned. Ontogeny does not repeat phylogeny with anything like that degree of fidelity which would be required if it were to be the means of unravelling the tangled thread of evolutionary progress. And the observed static end results given by the structures of existing animals are capable of being produced in too many different ways, as we now know, to make possible any precise conclusions from the mere study of their form as to the dynamic course of events which led to their existence.

IV

When this fact had become evident and sunk deeply into the consciousness of the working biologists, the way was cleared for the beginning of the great movement towards modern general biology. It is an odd mischance of fate that Darwin, who is the real founder of modern general biology, should not have seen any of its fruits in the declining years of his life, but instead only an abortive development resting on a ridiculously unsound philosophy. When biology, at the very end of the nineteenth century, got once more on the right track (for much earlier in its history it had been there, and only got diverted by a bad philosophy as to how the problems of evolution could be solved) a new world was indeed opened to our vision. And the password to it was experimentation. To the working biologist organisms once more became living things, not desiccated or pickled corpses. I cannot recall that in my undergraduate days there ever was a *living* animal in the laboratory, with the exception of protozoa. Certainly none was ever studied in any but a thoroughly pickled condition. As one looks back now on those days he is horrified not alone at the tortuosity of the intellectual pathway by which we attempted to come upon a knowledge of life, but also at the awful waste of alcohol!

The keynote of the new biology was dynamic and its methods were, in the main, experimental. Each of the old disciplines took on a new life. Morphology became experimental morphology; evolution became experimental evolution; a new shoot, ecology, sprang up from the gnarled old root of the taxonomic tree; and in some sense as the crowning glory of the whole edifice, animal behavior and comparative psychology began to flourish and attain a respectability never enjoyed by the labors of the old-fashioned naturalist, who observed what he called the "habits" of animals and plants.

Since these movements I have named comprise nearly the whole of the major trends of biology in the twentieth century it will perhaps be worth our while to examine a little more carefully into the philosophy and significance of each of them. For on and out of them is to grow the biology of the future, with all the great advances in knowledge which it has in store.

V

Modern experimental morphology may fairly be said to begin with Roux. His philosophy may be summarized in this way: organisms are machines which in their operations follow the laws of mechanics. Their structures are as they are because of the operation of these laws upon the plastic and adaptable material of which they are composed. It is the task of developmental mechanics to discover the specific physical and chemical laws which determine the form of particular structures of the living body. On the whole the most feasible way to go about accomplishing this result is to observe the results which follow upon the experimental modification of the physical and chemical conditions which environ the embryonic de-

velopment of particular structures. Then in the favorable case we shall be able definitely to connect and correlate particular physico-chemical events with particular biological events in a causal way. We shall replace metaphysical speculation in the field of morphology with observed physical causation.

The results of the last quarter century have abundantly justified the faith of Roux and his followers in soundness of this philosophy. So close are we to the events themselves, however, that we cannot justly appreciate, I believe, the enormous significance of the advance in our knowledge of the fundamentals of biology which have come as the result of the labors in this field of a host of workers, under the leadership of Roux in Germany and of Morgan in this country. The important advances in this field have, in the main, come from these two countries.

The great activity in the fields of experimental morphology and developmental mechanics has also been in considerable degree responsible for the growth and healthy condition of another major trend in modern biology, namely cytology. This is pure morphology at its best, resting on the sound philosophical purpose of the exact description of the minute anatomy of the cell. In this field America has again been a leader. E. B. Wilson's book,' 'The Cell in Development and Inheritance," may well be said to mark an epoch, at least in American biology. The achievements of cytology in the last quarter century have been of no mean importance. This field of research, for example, has played the leading role in clearing up the age old problem of the determination of sex. The discovery by McClung of a mechanism in the germ cells, the accessory or sex chromosomes, and the subsequent great extension and solid grounding of this knowledge by Wilson and his students, have served to take out of the realm of mysticism and put into the clear light of ascertained fact the answer to one of the great biological riddles. Again, in this same period cytological research has laid the structural foundation of the mechanism of heredity. The student of the history of science will note here an interesting fact. Discoveries of major importance in regard to dynamic biological events have here been made by a purely static, descriptive mode of research. This is unusual. Why it has happened so fortunately is because the American workers in cytology, in the period of which we are speaking, have at every stage worked in the closest touch with the experimentalists, and have directed their descriptive studies to problems which have made themselves compellingly obvious from and in the experimental work which was going on at the same time, and in many cases in the same laboratory. A static method has worked in correlation and cooperation with a dynamic experimental method. We see beautifully exemplified here one of the main functions of descriptive science in general, in relation to experimental science. The descriptive worker endeavors to lay the structural foundation of the dynamic events with which the experimentalist directly concerns himself. The fruitfulness of this method and ideal of work in morphology, as compared with sad sterility of the point of view which vainly attempts to solve *in toto* dynamic problems by a purely static mode of research as the older morphology did, is apparent in the recent history of biology.

VI

Jennings has somewhere said that "An animal is something that happens." While this happy phrase might well be taken as the slogan for all modern biology, it expresses with particular aptness the point of view of that major trend in recent biological history in which its author was the one of the most considerable pioneers and leaders, namely the study of animal behavior. The development of this subject into the prominence it has enjoyed in the last quarter of a century does not represent altogether quite so sharp a break with the philosophy of an earlier time as was the case in the development of experimental morphology. The field naturalist had always properly esteemed the importance of things which happened, and there exists, in the older literature of popular and amateur natural history, a considerable mine of rather accurate observations about the behavior and habits of

animals under natural conditions. Perhaps some day students of animal behavior from the modern view-point will adequately work this body of ore. It will not be an easy, nor a completely profitable task. The trouble of course is that, generally speaking, the naturalist of the old school was not analytical, but rather anecdotal, in his interest in the behavior and habits of animals.

It was just this difference that marked off the new school of animal behavior from the old. If what living things do is the most important consideration in distinguishing them from non-living things, it would seem clear that our knowledge of biology in general is bound to be increased if we apply to the study of what they do such precise analytical experimental methods as will give definite knowledge of at least some of the variables concerned in the determination of why they do it. In short, instead of interpreting what animals do in terms of a crude anthropopsychism why not be objective, and by experimentally modifying and controlling the animal's behavior learn something of the biological processes back of it?

Around 1900 it was pretty unanimously agreed that this was the thing to do, and it was done. For a few years a glib familiarity with "tropisms" and "reflex movements" was as essential to biological respectability as a corresponding acquaintance with "genes" and "crossing-over" is now. Two schools of thought and opinion crystallized, the one led by Loeb and the other by Jennings. They may be characterized, with perhaps the least chance of giving offense to anybody, as respectively the more simply mechanistic and the less simply mechanistic ways of regarding the happenings called life. The two cohorts of followers fought and bled on the battle-fields of "forced movements," "trial and error," and so on, with the utmost nobility and sacrifice of ink.

Quite unfortunately, as it seems to me, this fundamentally important line of research so brilliantly inaugurated, began after a decade or so to languish. Loeb turned off to physical chemistry and Jennings to genetics, and with the generals gone the armies melted away, to ally themselves to what they supposed to be more auspicious, or at least more fashionable movements. The case well illustrates the potency of the sheepish elements in human behavior. For no informed person supposes for a moment that all the problems of animal behavior and comparative psychology have been completely solved. Quite on the contrary the field has just been well opened up. And it is my conviction, based on some personal experience, that there is no other discipline which gives the student such an insight and grasp of fundamentals in the philosophy of biology as does the first-hand study of animal behavior. Every student in training for a career in any field of biology will find it extremely valuable in his future work to have done a piece of careful work in animal behavior under competent direction and guidance.

VII

We come now to the consideration of what, directly and in its numerous ramifications, is the dominant mode in present-day biology. I refer, of course, to experimental evolution. Beginning philosophically as a reaction against the sterility of pure morphology as a method of solving the great problems of organic evolution, it owes its actual origin as a major movement in biological thought to two circumstances, first, the bringing to light of the long-forgotten papers on the mode of inheritance of characters in certain plants by the Austrian monk, Gregor Mendel; and second, to the inauguration of the biometric method in biology by Francis Galton, Karl Pearson, and W. F. R. Weldon. It was plain enough to the writers of the Neo-Darwinian school, as indeed to everybody else who had grasped anything of the meaning of Darwin's work, that the basic factors in organic evolution were variation and heredity. Why not, then, study these factors directly, intensively, experimentally, and quantitatively? There could possibly be but one sensible answer to this question. And because this is so is the reason that genetics and biometry came upon us with such a rush, and have grown and prospered so vigorously.

Bateson, in the address to which I have already referred, tells the story of this change

in viewpoint in the study of evolution very well, and I cannot do better than quote him again:

Discussion of evolution came to an end primarily because it was obvious that no progress was being made. Morphology having been explored in its minutest corners, we turned elsewhere. Variation and heredity the two components of the evolutionary path, were next tried. The geneticist is the successor of the morphologist. We became geneticists in the conviction that there at least must evolutionary wisdom be found. We got on fast. So soon as a critical study of variation was undertaken, evidence came in as to the way in which varieties do actually arise in descent. The unacceptable doctrine of the secular transformation of masses by the accumulation of impalpable changes became not only unlikely but gratuitous. An examination in the field of the interrelations of pairs of well characterized but closely allied "species" next proved, almost wherever such an inquiry could be instituted, that neither could both have been gradually evolved by natural selection from a common intermediate progenitor, nor either from the other by such a process. Scarcely ever where such pairs co-exist in nature, or occupy conterminous areas do we find an intermediate normal population as the theory demands. The ignorance of common facts bearing on this part of the inquiry which prevailed among evolutionists, was, as one looked back, astonishing and inexplicable. It had been decreed that when varieties of a species co-exist in nature, they must be connected by all intergradations, and it was an article of faith of almost equal validity that the intermediate form must be statistically the majority, and the extremes comparatively rare. The plant breeder might declare that he had varieties of Primula or some other plant, lately constituted, uniform in every varietal character breeding strictly true in those respects, or the entomologist might state that a polymorphic species of a beetle or of a moth fell obviously into definite types, but the evolutionary philosopher knew better. To him such statements merely showed that the reporter was a bad observer, and not improbably a destroyer of inconvenient material. Systematists had sound information but no one consulted them on such matters or cared to hear what they might have to say. The evolutionist of the eighties was perfectly certain that species were a figment of the systematist's mind, not worthy of enlightened attention.

Then came the Mendelian clue. We saw the varieties arising. Segregation maintained their identity. The discontinuity of variation was recognized in abundance. Plenty of the Mendelian combinations would in nature pass the scrutiny of even an exacting systematist and be given "specific rank." In the light of such facts the origin of species was no doubt a similar phenomenon.

Now while it is true that genetics has by no means solved the problem of evolution as yet, and probably by itself never can and never should have hoped to, the intensive pursuit of this line of inquiry during the last decade has enormously advanced our knowledge of general biology. In the first place, thanks to the brilliant work of Morgan and his students with *Drosophila*, we have firmly welded the last links in the chain of a definite proof of the causal connection between particular visible details of nuclear structure in the germ cells and particular somatic characters transmitted from parent to offspring in inheritance. The "mechanism of heredity" is no longer a thing to speculate and build broad nebulous hypotheses about. We definitely *know* a good deal about this mechanism and how it works.

In the second place genetics, with cytology as a working partner, as we have already noted, has solved at least in broad outline, the problem of the causation of sex. In the third place, the general results of modern genetic study taken as a whole, and particularly the intensive study of the breeding of animals and plants which the getting of these results has entailed, have made it highly probable, as I think most geneticists, at least, will agree, that natural selection as postulated by Darwin, has had but little if anything *directly* to do with the causation of the evolution of the living things about us. That natural selection is a process always and everywhere going on in nature (except in the case of civilized man, where its operation has been in large degree suspended by virtue of certain attributes of civilization itself) no competent observer of nature can possibly deny. But that it either does or could bring about evolutionary results attributed to it by Darwin seems in the light of our present knowledge, indefinitely more improbable than it did twenty-five years ago. To give all the reasons which exist to support this view would be

wholly impossible with my time limitations. But that these reasons have been convincing to a great number of the most distinguished students of biology in recent years is certain. Because some of them have frankly given expression to their doubts, has led many well-meaning, but wholly uninformed, and somewhat unintelligent, persons to conclude that leading biologists no longer "believe in evolution." Nothing could be more hopelessly wrong than this conclusion. Every biologist who has got beyond a first elementary course in the subject knows that organic evolution is an observed and observable fact of nature, of something like the same obviousness and certainty as the fact that unsupported pieces of matter fall to the earth. I suppose that no one, even a "Fundamentalist," would think of asking a physicist if he "believed in gravitation." It is equally absurd to ask a biologist if he "believes in evolution." But just as one may appropriately discuss today the relative merits of Newton's and Einstein's views as to certain phases of the problems presented by the phenomenon of gravitation, so may he with propriety debate the significance of Darwin's theory of natural selection as a causative agent in the phenomenon of organic evolution.

It must seem to a young man or woman embarking now upon a career in biology that the only thing in the subject of any particular importance is genetics. I wish to point out, with a gravity as becoming as it is difficult to maintain while emitting such a platitude, that this is not true. There is a great deal in biology about which we are abysmally ignorant which partakes neither of chromosomes, nor Mendelism, nor yet of "crossing-over." And, if I mistake not, little light is likely to be shed on these dark places by the just now so brilliantly flaring torches that I have mentioned. The advancement of biology has at least one point in common with another fascinating subject, the adornment of women. Both progress evolutionally by a series of waves of fashion. Just now genetics is the reigning mode in biology. Nothing could be more charming, but it is neither the only nor the final word in charm.

It is apparently hopeless to expect anything like a reasonably balanced development in biological research, and, in consequence, of teaching. And perhaps if we had it we should all be bored. But it can do no harm if we think once in a while about some of the fundamental problems of biology which practically no one is even making an attempt to investigate experimentally, and towards the solution of which we are apparently making little progress. Time will not permit to say all that I should like to on this point, but I feel that I must in some degree indicate that what I have just said about the inadequacy of genetics as at present pursued, is not merely an idle gibe. To this end I shall discuss briefly two matters, adaptation and heredity.

The really difficult problem of evolution is adaptation. The original student of adaptation as a biological problem was Lamarck. It was the problem that lay behind and beneath all of Darwin's work, and he was almost the last investigator who in any systematic way busied himself with the problem. It seems to me that there are only two later students of this problem whose work is of very considerable importance, Hans Driesch and Lawrence J. Henderson. There is an objectively manifest teleology in animate nature. No thoughtful person can fail to be deeply impressed with the ingenuity and beauty with which organisms and their parts are adapted to the attainment of certain ends beneficial to the individual and the race. How came these adaptations about? What is the explanation? In the principle of natural selection Darwin put forward the first and, so far, the only mechanistic explanation of adaptation, though to Hume not Darwin should be given the credit of origination so far as this particular phase of the problem is concerned. It took away, if correct, at one stroke any necessity for the operation of supernatural causes in the explanation of the living world. It was this aspect of Darwin's theory of natural selection which disturbed thoughtful theologians vastly more than the fact of evolution itself, the descent of man from lower animals. For it was and is always possible, even if not plausible, to argue that the Creator chose to work in an evolutionary manner in the building of the world. But a strictly mechanistic

explanation of adaptation, if adequate, destroys completely the very keystone of the arch of any theistic philosophy. Nothing could undermine more completely the prestige of a theistic agency than to prove that it is unnecessary—than to show, in short, that the supposed results of its infinite wisdom and omniscience not only would have occurred, but actually did happen as a result purely of natural, mechanical causes without any external, supernatural intervention.

The question, however, is: did the manifold adaptations which we see in living nature in actual fact arise through the operation of the processes of trial and error and natural selection? A final answer to this question seems to me impossible in the present state of knowledge. In the eighties and nineties the answer would have been, among biologists if not among philosophers, almost unanimously affirmative. Today the case seems much more doubtful. *Formally* it is possible to explain many particular adaptations by natural selection. Some it appears impossible to explain in this way, even formally. What wants intensive investigation is the whole biology, from every conceivable angle, of *particular* adaptations. No more important problem exists. And its difficulty should act as a stimulus rather than a deterrent to its study. To solve it, or indeed to contribute significantly to its solution, will require a different point of view and a different method from that of present-day genetics.

It may seem a little ungracious to suggest, in view of the brilliant results of genetic work which I have already mentioned, and which I yield to no one in admiration of, that the present dominant mode of research in genetics can give us only an incomplete and, philosophically considered, somewhat superficial knowledge of heredity, but I am unable to convince myself that such is not the fact. My views on this point have not changed since I discussed it in detail some seven years ago. I then said[6]:—

Mendelism finds its limitations, just as did the biometric methods in the fact that from the logical standpoint it is essentially a statistical method which studies only the laws of distribution of things given or assumed. It examines only the distribution of hereditary specificities, and not at all, directly, their origin or determination. The former aim cannot be the goal of genetic science. A method which can travel only so far cannot hope to say the last word in the discussion of the problem of heredity. As a mode of research the Mendelian method of analyzing the progeny distributions rather than the ancestral will always be used. It was indeed one of the most brilliant methodological discoveries in the history of science. But it has limitations in the direction of what it can accomplish per se in elucidating the problem of heredity.

It is altogether usual in current discussions of variation and heredity to neglect completely everything which comes between the two end terms of the ontogenetic series, the germ cell on the one hand and the adult soma on the other. But clearly what goes between is a most essential part of heredity itself. It is astonishing how little has been done on these extremely obvious problems.

Two of the four general methods which have been employed in the investigations of the problem of heredity have been seen to be essentially statistical, and two essentially biological. The statistical methods—the biometric and the Mendelian—differ fundamentally only in that the former investigates primarily the ancestry and the latter primarily the progeny. Logically exactly the same distinction was found between the two purely biological methods—the cytological and the embryological. The former studies the ancestry of the germ cell (gametogenesis), the latter the progeny of the germ cell (somatogenesis).

All of these methods are valuable, and each has contributed to our present knowledge of heredity. No one of the methods alone can, however, solve the problem. They all have at least one fundamental limitation in common. This is that they offer no means of directly getting at any definite information regarding the origin, cause, or real nature of that specificity of living material which is the very foundation of the phenomenon of heredity. The distribution of hereditary specificities, their putative morphological "bearers," and many other things about them have been studied more or less exhaustively. The things themselves have been speculated about, but not investigated to any but the slightest extent.

6 Pearl, R. Modes of Research in Genetics. New York (Macmillan), 1915.

VIII

In bringing to a close this brief and inadequate review of the major trends of biology I want to say a few words about a purely practical movement which is rapidly gaining force and seems likely shortly to have a pronounced effect upon the development of the whole subject, including its theoretical aspects, and particularly its teaching. I refer to the rapidly growing recognition of the fact that all of the activities of all living things, including man, are properly a part of biology in a greater or less degree. The practical importance of this lies in its corollary that the biologist may and probably does have something important to contribute towards the solution of the most various sorts of human problems, agricultural, medical, social, economic, and so on. During the last quarter of a century it has been increasingly forced upon the attention of university teachers of biology that students of sociology, of philosophy, of medicine, of economics, and of many other subjects, who had no intention to become professional biologists, not only wanted to, but needed to know something about biology. At first covertly resisted, this need is now frankly being recognized and in some degrees met by the reorganization of courses, and departures of varying degree from the traditional method of teaching this subject. This is, I think, entirely healthy and desirable. There is going along with this broadening of the viewpoint of biological teaching a welcome broadening of the opportunities for a useful and profitable career in biology. There are already many kinds of applied biology attracting young men and women. And quite beyond the range of these somewhat narrow specialties, we are witnessing such phenomena as the employment of research workers in general biology by a great corporation manufacturing electrical appliances, to mention but a single instance.

To one who embarked upon a biological career twenty-five years ago, solely because he was seduced by the charm of the subject, and who in yielding renounced, against the advice of family and friends, the supposedly certain and considerable rewards which would come if he continued, as he had tentatively started, on a career in which he might finally become a teacher of Greek, the opportunities for the biologist of the present day seem somehow humorously magnificent.

If in what I have said I have succeeded in any degree in indicating the intellectual justification of Dr. John A. Lichty's splendid gift to Mount Union College for the endowment of its flourishing department of biology, my principal object will have been achieved. Under the able leadership of Professor M. J. Scott we may confidently expect the work of the department to go forward in close touch with each new and promising field of endeavor which biology presents. I can not allow myself to close without expressing, as a biologist, my deep admiration and profound respect for the breadth of vision and deep philosophical insight which is implied in the endowment by a worker of the field of medicine of a chair of general biology. The Milton J. Lichty Chair of Biology is another enduring demonstration of the fact that the most enchanting of all the sciences has really come into its own.

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EARTH-CURRENT OBSERVATIONS[1]

THE Department of Terrestrial Magnetism of the Carnegie Institution of Washington is planning to install earth-current lines for systematic observations at its magnetic observatories. During this year such lines are being installed at the Watheroo Magnetic Observatory, about 120 miles north of Perth, Western Australia, and some time later similar installations will be made at the Huancayo Magnetic Observatory, about 125 miles east of Lima, Peru; both of these magnetic observatories are conducted under the auspices of the Department of Terrestrial Magnetism. Various initial investigations concerning best methods of earth-current

[1] Presented before the Philosophical Society of Washington, February 25, 1922. The full paper is published in the March-June, 1922, issue of *Terrestrial Magnetism and Atmospheric Electricity*, pp. 1-30.